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MEMORY AND THE ECONOMY OF LEARNING.<sup>1</sup>

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Since Ebbinghaus published his monograph *Ueber das Gedächtniss* in 1885, the study of memory has taken a prominent place in experimental psychology. The method of reproduction which Ebbinghaus used has proven itself most valuable in broadening out and correcting results which hitherto had been obtained by introspection alone. This method is very simple, consisting in the presentation of an object and group of objects and the voluntary reproduction of the same after a fixed intervention of time.

In order to work out a concrete problem one must, of course, following the dictates of all experimental investigation, seek first the simplest *material* to work with and then control all the various *conditions* as carefully as possible. Ebbinghaus found his *material* in nonsense syllables which he arranged from vowels between two consonants into series of different lengths. In order to keep the *conditions* constant he experimented only on himself, being careful to work at stated hours of the day and in a certain mental readiness. The series at hand was then repeated aloud at a fixed tempo until learned by heart. The *number of repetitions* and the *time* required for the learning were both carefully noted. In this way many similar series were learned on different days, and then relearned after varying intervals. The time and number of repetitions saved by the second learn-

<sup>1</sup> Read before the Western Philosophical Association in Columbia, Mo., April 1, 1904.

ing over the first gave a direct measure for the parts retained in memory during the intervals of the two learnings.

By varying the length of series and interval many interesting relations were found. Thus, for example, a series up to 7 syllables in length could be learned after a little practice in 1 repetition. 12 syllables, however, required 17 repetitions on the average, 17 syllables, 30 repetitions, 24 syllables, 44 repetitions, and 36 syllables, 55 repetitions. That is to say, the increase in the number of repetitions required is at first proportionately greater than the increase in number of syllables; later it becomes less. As to the influence of the time interval on retention, it was found that memory fails rapidly during the first hour but less so during the next succeeding hours until, when a day is passed, the parts still retained fade thereafter very gradually as time goes on. Even after a period of 22 years Ebbinghaus was able to detect a saving of 7 per cent. by the relearning of a portion of Byron's 'Don Juan,' which in the meantime he had not seen. Since the appearance of Ebbinghaus' work the number of investigations in this field has been so great, the problems attempted and solved have been so manifold, it begins to seem worth while to pause a moment on the way and 'take stock.' Ebbinghaus himself, in his *Psychologie*, has done this in a quite satisfactory manner for the work up to 1902. And more recently Otto Lipmann has given us a short account of the practical results which have accrued from the experimental study of memory.

In the brief time at my disposal this morning I should like to invite your attention to a few points in regard to the economy of learning.

One of the first considerations in drawing conclusions for economy in learning must be a strict analysis and differentiation of *types of learners* and *ways of learning*. There are three main factors which go to form our fundamental type distinctions: visual, aural and kinæsthetic. The normal individual possesses all three in varying degrees. The predominance of one over the others determines the classification as the visual, auditory or kinæsthetic type of person. But by far the larger number of persons are of a mixed type which often seems to defy very

close analysis. So Netschajeff, in studying the types of 700 students, between the ages of 11 and 19, in a Russian military school, found only 11 per cent. whom he could refer definitely to one of these three classes, though 49 per cent. combined two factors to neglect the third. Of these, 32 per cent. were visual-kinæsthetic, 12 per cent. visual-auditory and only 5 per cent. auditory-kinæsthetic. The last is a rather unexpected result, since we have been inclined to hold auditory and kinæsthetic factors (evidenced in movements of the lips and throat) as fairly inseparable. The remaining 40 per cent. of Netschajeff's subjects were indeterminate.

Aside from this fundamental analysis of the mind's working material, it is also of importance to distinguish an intellectual and a sensory type of person in accordance with the mind's manner of making use of its factors. Under the intellectual or logical type I should class persons who, with very slight play of imagination, grasp the matter presented *as it is*, holding to its objective factors, adding very little of a subjective nature. The associative connections which serve to bring the thing into the mind are such as will give it meaning and are all checked by a sort of *mental inertia*, i. e., by more or less clearly defined tendencies to persevere along certain lines of thought. Accordingly such stray suggestive elements as might call up reproductions not directly bearing on the matter at hand are avoided.

The sensory type of person, on the contrary, is rather subjective than objective in nature. He reproduces readily on the presentation of any motive and is highly associative. Sense perceptions as such rather than abstract concepts mean much to him. His mind works rapidly, intuitively, where the intellectual works slow and carefully.

As to the ways of learning, we are confronted with a *slow method* and a *fast method*. The intellectual type of person favors the slower method. The very careful consideration and painstaking observation of this type usually demands, in learning, a greater number of repetitions and a comparatively slow rate of speed. The sensory type of person learns faster, as a rule. His learning is based much more intimately on the sensory factors themselves. The quickness of his learning is

due to the more or less automatic and intuitive way in which he grasps the whole and neglects the parts.

For the learning of nonsense syllables there are two distinct rates of speed which, with respect both to expediency of learning and retention in memory, show themselves more advantageous than all the rest. The first is a comparatively slow rate at which the syllables follow one another in approximately one and one half second intervals. This represents the slow method of learning. The other, representing the fast method, is three times as quick, *i. e.*, syllables follow in one half second intervals.

Meumann and Stern have been inclined to think that practice would level the differences of fast and slow learners, that probably the fundamental capacity for learning is the same in all persons. On the results of certain experiments with school children Meumann concludes that with sufficient practice all persons would most probably learn equally fast at a uniform rate of speed. The seeming differences of individuals, he thinks, lie in the varying length of time requisite for adaptation to the work.

There can be no doubt but practice does exert a levelling tendency of this sort, yet I can scarcely believe it possible to bring all learners to the same method and results unless type distinctions be at the same time obliterated. In my own experiments, though the influence of practice was constantly noticeable, still the slow learner of intellectual type persisted throughout in requiring a greater number of repetitions and a longer time than did the fast learner of sensory type.

To turn now for a brief consideration of these two rates of speed mentioned; the slower was found to be generally most advantageous from the point of view of a small number of repetitions. The faster one gave the shortest time for learning. How, then, shall we conclude as to the relative advantages of these speeds for different persons? We are not justified in considering either of these factors without the other. Ebbinghaus makes a tentative conclusion in favor of a fast rate of speed for all, because he found himself thus able to learn and relearn his matter with the least expense of time. The increased number of repetitions attending the faster tempo was left out of consid-



eration. But since every increase of this sort must mean an increased expenditure of energy, economy requires that we take the number of repetitions into account. It follows that the ideal tempo should be such that one might learn with expediency and also without too great an effort. The organism as a whole can best afford to make a compromise in favor of that rate of speed where the best combination of learning time and repetitions may be effected. This point of view controlled the selection of the two rates of speed noted, and was applied both to the primary learning and the relearning. The slower of the two is best adapted to the logical method of the intellectual type where a full consciousness of every factor is desirable. The faster rate is one for a sensory type of person whose attention is directed on groups and the total impression rather than on single factors.

It must be added that these two rates of speed do not hold good for sense material, though the methods do. The factors which go to make up sense material are of unequal importance. The difficulties are not so great—particularly those of articulation. Consequently all persons tend to select a faster and more uniform rate of speed which varies from 0.3–0.4 second per syllable. A person of intellectual type ordinarily finds no hindrance to his method at such a speed; and, as to the sensory type, articulatory difficulties make it impossible for him to make use of a faster rate. Individual differences of method do not change. Variations in ease of learning and powers of retention are the same as in the learning of nonsense syllables.

The most practical application of these results is to be found in the school room. All teachers know the difficulty they meet with in directing the study of some pupils. Among these there is the one who leaves his lesson until the last half hour and then with increased energy attempts by faster reading to impress the matter on his mind in the brief time at his disposal. The teacher is usually at war with this sort of pupil even though he may make good recitations. "Quick learning and quick forgetting go hand in hand," he is told, and his methods are accordingly discouraged. Our experiments justify us in doubting the entire wisdom of this dictum. Quick learning is not

necessarily followed by quick forgetting. Provided the student *learns* his lesson his method is not at all so poor. It doubtless represents his mental type and, if so, he will probably retain the matter better when learned in this manner than if he had spent two hours over it with his mind 'wool-gathering' during three fourths of the time.

On the other hand, we have the evils of 'cramming' which are potent enough and must, naturally, be considered in connection with rapid learning of this sort. Müller and Pilzecker have pointed out that our perceptions require a certain time to 'set,' to become organized, before they can be effective in memory. Without allowing this period, dissociation results which leads to a partial or total loss of memory. Burnham has recently made use of this fact (though curiously enough without reference to Müller and Pilzecker) in advancing a theory for retroactive amnesia. It is very harmful to the associative connections just established, when one turns from a concentrated study of one sort directly to something else which requires equally attentive consideration. This is what the student does when he 'crams.' He impresses his mind with a number of more or less diverse and disparate facts without supplying those associative connections which might systematize them. Consequently all are impressed in a quite mechanical fashion. Later he reacts automatically to whatever motive the teacher's question may give. When the proper motive is lacking — if the teacher puts his question in an unexpected form — the answer fails.

It is important that the teacher should study his pupils more carefully. Psychology shows that the doors to the mind are not all of the same size. Different individuals require different presentations in order that they be appealed to in a like degree. Not only is this true of different individuals, but of the same individual at different stages of his development. It is generally known that a child is highly suggestible up to about his fourteenth year. Pearce has demonstrated that up to this age the degree of suggestibility is, in general, directly proportional to the child's intelligence. A stupid child is much less suggestible than a bright child. This is the time when the individual is 'taking in the world,' learning to coördinate his sensations and

reproductions and to respond with adequate movements. Above this age the mind ceases to mirror all that comes. Certain impressions become more favored than others. Habits are formed. The child begins to develop a character. The degree of suggestibility decreases with increased age.

With this in mind we may return for a moment to our type distinctions. All children are sensory in type. It is meaningless to speak of an intellectual type of child. This being so, it would be well to study first the child's mental furniture: his visual, auditory and kinæsthetic factors. All mental improvement should be along the line of developing the child's skill in making use of these factors. It is quite thinkable that the careful development of a purely mechanical quickness in perception and repetition, by causing the child to learn things by heart at gradually increasing rates of speed, might prove of the greatest value to him as he matures.

The teacher should have a pretty exact knowledge of his pupils' types and tendencies. He will find both quick and slow pupils; the quick may be bright or they may be automatic; the slow may be thorough-going or they may be stupid. These analyses once made, it should be possible for the teacher to so direct the study as to gradually overcome the harmful tendencies, to preserve and strengthen the good ones. A pupil who tends to be mechanical might be directed into a more intellectual method by being made to learn at a slow rate of speed where his mechanical helps are unavailable. The dull pupil might be quickened by a fast learning which supplies a certain mechanical stimulus and requires, at the same time, an alert and attentive state of mind. In the plastic, suggestible state of childhood, before the character is firmly welded, it may be possible to mould and so turn to better account the fundamental factors which the child possesses. When adolescence has come certain tendencies are manifest. Further development of the memory, in order to be effective, must be along the line of these natural tendencies, not opposed to it. It must not be thought that the intellectual type of learner is superior to the sensory type. These merely represent two sorts of persons each acting in accordance with his natural disposition. We

should then, as teachers, study the pupil with a view to training and developing what we find already in him, rather than attempt to enforce an unnatural method or to supply something which natural endowment has failed to provide.

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## PSYCHOLOGICAL LITERATURE.

### VOLUNTARISTIC PSYCHOLOGY.

*Die Grundlehren der Psychologie vom Standpunkte des Voluntarismus.* NIKOLAJ LOSSKIJ. Uebers. von E. KLEUKER. Leipzig, Barth, 1904. Pp. 221.

This book presents a consistent and a relatively new conception of psychology. So far from regarding psychology, after the traditional fashion, as science of all phenomena of consciousness, it deliberately bars out one group of conscious contents—given states of consciousness—from the realm of psychology. As material for psychology, it admits only such experiences as are recognized as 'mine,' and it defines psychology<sup>1</sup> as 'science of the subjective world, that is, of the totality (Inbegriff) of my conscious condition.'

But this limitation of the field of psychology is far less significant than the positive doctrine of the book concerning the nature of those conscious contents which by virtue of being 'mine,' are rightly the concern of psychology. These are conceived by Losskij as acts of will, consisting fundamentally (1) in conations (Strebungen),<sup>2</sup> supplemented (2) by the feeling of activity, that is to say, the feeling of the dependence of certain phenomena on our conations,<sup>3</sup> and ended (3) by some change, inner or outer, a fact of consciousness or a bodily movement.<sup>4</sup> Psychology is thus, in Losskij's view, voluntaristic and personal. The phenomena which it investigates are, on the one hand, volitional activities; and, as such, they are of necessity also acts of an 'I.'

Midway between sensations, which are always involuntary, or given (and thus extra-psychological phenomena), and choices, which are obviously appropriated as 'mine,' lie a series of conscious phenomena which may be either 'given' experiences or 'mine.' "Certain phenomena," Losskij says, "which in content are perfectly homogeneous, for example, the passionate impulse to drink wine and the actual drinking, may belong now to 'my' subjective world, now to the 'given' objective world."<sup>5</sup> Percepts and images, as mainly sensa-

<sup>1</sup> 5. Kap. V., III., S. 139.

<sup>2</sup> 1. Kap., S. 4.

<sup>3</sup> 1. Kap., S. 17.

<sup>4</sup> 1. Kap., I., S. 25-26.

<sup>5</sup> 4. Kap., II., S. 122.



tional, belong rather to the class of 'given' states of consciousness; yet, as discriminated and attended to, they too become 'mine.' For, as Losskij insists,<sup>1</sup> it is possible that "one and the same phenomenon, in so far as it is experienced as 'mine,' contains elements of the will-act, yet, in so far as it is experienced as 'given,' does not fall under the concept of the will-act." Pleasantness and unpleasantness are examples of experiences which may be 'mine' —as, for instance, pleasure in the solution of a problem or in the fulfillment of a hope<sup>2</sup> — but which may also constitute a 'given' experience — as, for example, the unpleasantness of the touch of an icy object. Even one's most productive consciousness may be 'given,' forced upon one from without; for example, "the unexpected explosions of creative thought in the moment of producing a work of art or a scientific discovery usually perfect themselves as if a higher genius suddenly were given a whole new world of thought."<sup>3</sup> Emotions, on the other hand, are counted by Losskij among the active and personal experiences, characterized by conations and by the feeling of activity.<sup>4</sup>

The central concepts of the system are evidently then the concept of the 'I' and that of the will. The second paragraph of this review reproduces Losskij's analysis of will as given in his first chapter. He devotes his fourth chapter to the discussion of personality. After pointing out that both sensations and the involuntary thoughts which 'flash upon the periphery of consciousness' are admitted to be unrelated to the 'I,' Losskij adds that even conations "may stand very near the 'I,' yet are not the 'I' itself \* \* \*. Obviously," he says, "the 'I' is something higher than the single conations; yet it is not more nearly definable, \* \* \* and appears to us only in the form of the unity of our conations. \* \* \* The most notable character of this unity is its self-dependence \* \* \* in the sense that its elements \* \* \* have their source in the 'I' instead of building it up."<sup>5</sup>

From even so slight a summary, it is evident that this book concerns itself with fundamental theory, not with detailed problems, and that the criticisms of its teaching will vary with the standpoint of the reader. The present writer finds the greatest value of the book in its reiterated teaching that the recognition of an 'I' is a fundamental demand of science;<sup>6</sup> and its greatest defect in the doctrine that the

<sup>1</sup> I. Kap., I., S. 32.

<sup>2</sup> 6. Kap., I., S. 143.

<sup>3</sup> 4. Kap., II., S. 121.

<sup>4</sup> 7. Kap., S. 177 u. S. 184.

<sup>5</sup> 4. Kap., I., S. 112.

<sup>6</sup> 4. Kap., I., S. 116.

elements of personality are exclusively active and volitional. On the other hand, in the opinion of this reviewer, there is no experience, even perception or imagination, in which one is not at least obscurely conscious of oneself; and this self-consciousness is not always an activity, but may be a mere receptiveness. Thus, every conscious experience is 'mine';<sup>1</sup> yet, conversely, every conscious phenomenon, volition as well as image, may be looked upon, by an artificial abstraction from its relation to a self, as a given content.

It is to be regretted, in the second place, that Losskij does not sufficiently work out a doctrine of the social nature of that self on which he lays such stress. To be sure, there are not lacking indications that he grasps this truth; in particular, he explains the fact that I am conscious of contents which, as 'given,' are yet not 'mine,' by the hypothesis that these given facts of consciousness belong, directly, to some other 'I,' either to a subliminal self, whose physical condition is a lower nerve-center in my own body,<sup>2</sup> or to a physiologically, as well as psychologically, different individual self. The immediately realized unity of different selves is well illustrated in the following passage: "Almost every lecturer, teacher or preacher has experienced moments in which a whole audience listens with bated breath and becomes transformed into a single being which is immediately connected with his own being."<sup>3</sup> But Losskij is content to suggest the possibility of this essentially social nature of the 'I' and its interrelation with other selves, instead of making it, as he well might have done, an integral part of his teaching about personality and a basis for the classification of conscious experiences.

These criticisms concern themselves with Losskij's conception of the 'I.' On the side of will, the most vulnerable portions of his teaching are, in the opinion of the writer, his doctrine of conation as a distinctive experience, 'ein eigentümliches, unzerlegbares Gefühl des Hindrängens,'<sup>4</sup> and his treatment of emotions as volitional phenomena.

So brief a notice does not do justice to the merits of the book. It is admirably written, is full of close analysis, and fresh illustration, and anticipates most of the criticisms made upon it. Besides these general advantages, it has many excellencies of detail. For example, it lays stress on the useful antithesis between 'ungewusst' and 'unbewusst';<sup>5</sup> it includes an admirable critique of associationism;<sup>6</sup> and it

<sup>1</sup> Cf. 1. Kap., II., S. 29 seq., for a consideration of this objection.

<sup>2</sup> 4. Kap., II., S. 125.

<sup>3</sup> 5. Kap., II., S. 135.

<sup>4</sup> 1. Kap., I., S. 5.

<sup>5</sup> 2. Kap.

<sup>6</sup> 3. Kap., II.

presents an excellent summary and criticism of the James-Lange theory of emotion.<sup>1</sup>

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### RHYTHM.

*Researches on Rhythmic Action.* ISHIRO MIYAKE, Ph.D. Stud. Yale Psych. Lab., 1902, X., 1-48.

This thesis shows evidence of the expert hand of the technician. It wrestles with old problems, but has applied more accurate and suitable apparatus to their solution. These problems are: the possibility of voluntary arhythmical action; the influence of auditory and visual stimuli on regulated and free rhythmical action; the effect of emphasis on the length of rhythmical intervals in noiseless tapping, arm movements with sound and in vowel scansion, and on the pitch of the vowel; and the determination of the point where the finger beat occurs in the course of different vowels scanned in unison with the beats. About three years were spent on the investigation, which is commendable for its fulness of experimental details. One might venture to express the wish that more subjects had been used in some experiments. Valuable results are contributed in each division. Space limitation makes it necessary to confine our critical analysis to certain points of most interest, which may still require modification and supplementing, and to point out certain omissions which make it difficult to reconcile the writer with himself on the basis of his own results, and which, when supplied, contribute additional facts of value. We proceed to the latter point first.

Dr. Miyake's omission consists only in a failure to give average results, and to compare these. I have taken the time to calculate them, with the following results:

1. As affects his statements. The following corrections are typical. In the pattern 1'-2-3, 2-3 is practically of the same length as 1'-2 (difference = .002 second), while the inter-group interval is actually *shorter* than the interval after the weak beat for the finger taps and drum beats, and the same length for the scansion, instead of the 'lengthening' being less 'remarkable' (p. 16). In 1-2'-3, 3,-1 (.638") is shorter than 1-2' (.647"), instead of being 'very constantly longer' (p. 17). Instead of the difference in the 'lengthening of the interval between the groups' being 'remarkable' for the 1'-2-3 and 1-2'-3 patterns they give precisely the same average (.638"). The conclusion which is drawn, however, that the length of the pause depends 'on the amount

<sup>1</sup>7. Kap., I.

of difficulty of the formation of the rhythmic group,' does apply to the patterns with the arm movement and the scansion. On the other hand, it is more in accordance with the results to say that the inter-group pause is lengthened in some patterns (*e. g.*, 1-2'-3), but shortened in others (*e. g.*, 1'-2-3), instead of the statement at the bottom of page 21. Why this should be so might be due to two reasons: (1) The difficulty of forming the pattern in the one case (Miyake); (2) the tendency of the interval from group to group to approximate to a representative, normal length of rhythmical period. There is evidence in favor of such a supposition; the pauses are longer in the two-beat groups than in the three-beat, with one exception: 2,-1' noiseless taps.

A study of the *rhythmical consciousness of the 'gallery'* shows that there is a certain time of interval in musical tempo which evokes a general response, which instinctively excites or releases the typically rhythmical expressions.

2. As affects additional contributions. (1) The group-interval and the interval from group to group (a group-interval + the following inter-group interval) are practically constant for patterns with the same number of constituents: *e. g.*, 1.232 and 1.283<sup>s</sup> for 1'-2 and 1-2', and 1.994 and 1.972<sup>s</sup> for 1'-2-3 and 1-2'-3, respectively. This holds fairly well for the hand-movement and scansion also. The length seems to be a function of the number of components; and this has also been found to hold for the centroid intervals in speech. So that while there may be an approximation to a rhythmical norm, the intervals will vary according to the "filling-in." This disproves a position reached by Brücke more than thirty years ago; and similar positions held by later writers (*e. g.*, Lanier; Bolton, *Rhythm*, p. 28). Still this may not apply to subjective rhythmization, where the length seems to vary inversely as the number of components (*cf.* Bolton, *Rhythm*, 70). (2) In general, the time for the two-beat group is between  $\frac{1}{2}$  and  $\frac{3}{4}$ <sup>s</sup>, or  $1\frac{1}{4}$  and  $1\frac{1}{2}$ <sup>s</sup> for the entire interval (group to group); and for the three-beat group about  $1\frac{1}{4}$ <sup>s</sup> (singularly the variations are exceedingly small), or from about  $1\frac{3}{4}$  to 2<sup>s</sup> for the entire interval. Even the two-group here is considerably longer than the most prevalent intervals in the arrhythmical hand movements. Have we here an analogue of respiration? It is slower and deeper as it is more automatic and rhythmical, and faster in moments of excitement. But a point of chief interest here to the reviewer is the illustration afforded anew of the danger of applying the results of artificial, laboratory scansion to the rhythm of free, spontaneous speech. (I must distinctly affirm, to prevent misunderstanding, that

no conclusion drawn by the author directly prompts this remark). The intervals in scansion appear to coincide pretty closely with the lengths of the periods used in motor activities in general in laboratory tests. But they appear to be considerably longer, with the single exception of the two-group, than the corresponding centroid intervals in natural speech, whether prose or poetry. It is a truism to remark that we cannot get along in the affairs of life with the slow, jog-trot speed of 'scansion.'

3. The lengths of the hand-movement patterns are shorter than either of the others. The time is thus not directly dependent upon the amount of movement or space travelled. The probability is that the execution of innervations of the fundamental muscles is quicker than that of the finer, accessory muscles. From this point of view the larynx movements, which we may regard as evolutionally accessory, become especially interesting. The speech intervals are noticeably longer. The only exception, trochee, has been considered by some to be the normal Anglo-Saxon metrical pattern, while others have argued for the iambic. Squire regards the former as genetically prior; Bolton makes it the type of subjective rhythmization (emphasis first); while Wundt considers both equally psychologically simple.

4. Miyake's trochee pattern is longer than the iambic, for the tap, drum beat and scansion. This is the direct opposite of Hurst and McKay's result, unless the inter-group interval is included, when the results agree for the finger-tap and scansion. But even thus the difference remains in the hundredth-of-a-second column, so that this moot question appears still to be an open one. In Triplett and Sanford's experiments there appeared to be no characteristic time difference between them.

5. To Bolton's conclusion that longer pauses precede the accented sounds, these results show twelve exceptions and no instance of confirmation. The centroid in speech also shows that the longer silence follows the emphatic syllable (see later); this indeed agrees with Bolton's result for duration emphasis. His results were founded on mere introspection, the others referred to on exact measurements. If we accept measurement in favor of introspection we have, it seems, an interesting case of illusion affecting auditory consciousness in Bolton's subjects—unless we prefer the alternative of saying the discrepancy is accounted for by differences in the experiments: the longer interval (pause or silence) occurs after the emphasis, but subjectively before it, especially with loudness emphasis.

Miyake founded his conclusions on individual ratios. The fore-



going results should be considered in the nature of a plea. Ratios furnish a fruitful means of estimating values, perhaps the most fruitful; but used to the exclusion of the average, errors are often, perhaps generally, inevitable. Several lines of evidence are preferable to one. Collating different mathematical expressions is laborious and makes the discussion appear technical, uninteresting, perhaps trivial and too spread-out, but to ignore this is to sacrifice precision, accuracy and fact.

Dr. Miyake's explanation of the greater prolongation of the inter-group pauses as serving the purpose of marking off the groups more clearly, commends itself to thought and is probably valid in many cases. Such pauses usually mark off verses in spoken poetry and phrases in prose, but they make speech rhythm essentially a discontinuously recurrent phenomenon; and, what is more, they are not the typical factor unifying centroid intervals. (I employ centroid here in the technical sense, previously defined, not of emphasis, but the highest point of intensity in the course of emphasis). In fact, measurements of speech intervals show that the old supposition of metrists of the compensatory character of the pause in the centroid interval is wholly groundless. The centroid is the supreme unifying agency. But, this apart, Miyake's results show that the inter-group pause may even be shorter; even in tapping its rôle as a segregating and unifying element must be subsidiary; it is not, apparently, of universal applicability. Thus the theory must be supplemented. In the pattern where the emphasis came last (1-2') the interval was longer. How shall we explain this? It will be helpful to refer to a general fact in speech. We must clearly distinguish between what may be arbitrarily called silences and pauses, the latter representing physiological and psychological resting stops, serving a purpose in the expression of thought, more or less conscious; the former corresponding to transitional processes of the larynx in passing from sound to sound, strictly unconscious. Now, what measurements we have made show that the three kinds of silences range in length, from longest to shortest, as follows: inter-, post- and pre-centroid silences. That the silence is longer after an emphatic syllable than before it is probably due to the greater effort needed to recover from a strong blow, or the higher degree of inertia following the latter. On our theory, then, the interval 2', 1 is longer because the subject *underestimates* it. He makes no allowance for the emphasis 2', *i. e.*, for the longer silence following it. This unconscious element is unconsciously added to the conscious (Miyake's 'pause'). On this theory Hurst and McKay's

result, that the iambic pattern is longer than the trochaic, would seem the more probable provided (1) the measurements extend from 1-2', - 1, thus embracing the post-centroid silence, or (2) if only to the end of 2', that the emphasis 1-2' is stronger than the emphasis 1'-2, which seems to be the case with me. If a pattern *scanned* as iambus were *measured* as trochee from centroid to centroid, it should accordingly become inordinately long, because it consists of the longer iambic centroid syllable and silence.

Finally, we notice that the theory advanced harmonizes with the fact that the inter-group pause may be actually shorter in the 1'-2-3 pattern. The silence is here added to an intra-group pause.

To the conclusion of the final lines I find it difficult to give unequivocal assent: 'the point of emphasis in rhythmic articulation lies at the *beginning* (emphasis Miyake's) of the movement of the vocal organs.' Taking the author's own results, that both sight and sound signals are anticipated, we must conclude that the beats anticipated the centroid in the syllables. Hence a correction must be introduced. All that we would be warranted to assert is, so far forth, that the centroid must be located *after* the beat, in the course of the syllable.

But suppose we accept the conclusion. Then we must clearly define what we mean by the highest point of emphasis. There are two kinds of centroids, the spoken and the heard. The beat may correspond with the centroid at the moment it is felt by the speaker, but not with the centroidal effect as experienced by the listener, which obviously comes at the point where the atmospheric vibrations attain their maximum effect on the cochlea — unless, indeed, the hearer is also subject to a complementary law of anticipation. This view, it seems, however, is inescapable if the experimenter's theory would be reconciled with his results, for he holds — as is now generally conceded — that pitch is a structural element of the centroid. But all his figures (Figs. 9-26) show that the pitch rises from the start and attains its maximum toward the close of the emphasized vowel (*a*). Of course the maximum centroidal effect of pitch may also be a departure downward, but this is less frequent; there is no instance in Miyake's emphatic vowels. It is even probable that in an utterance which is emphatic because occupying a low instead of high place in the pitch scale, the pitch will almost always follow the same direction in the course of the sound. But what I now would particularly emphasize is, unless this view of the centroid is accepted we must assume that the different elements are not synchronous, that the loudness element comes earlier than the pitch — a conjecture lacking both in natu-

ralness and experimental evidence. On the other hand, Brücke's results run squarely counter to Miyake's and Meyer's; the centroid occurred at the *end* of a short vowel or just before the following consonant; somewhere in the *course* of a long vowel not immediately followed by a consonant; and near the *boundary* between the long vowel and the consonant when the vowel does not terminate the syllable.

In fine: this problem cannot be attacked intelligently unless we distinguish between the centroid as produced and as received, as a motor phenomenon of the larynx and as a sensory phenomenon of the cochlea. The heard centroid must at least come so much later than the spoken as the rates of motor (speaker) and sensory (hearer) nerve transmission and air concussion require. The hearer will, so it would seem, lag slightly behind the speaker; the latter anticipates his emphasis; he must feel them before he utters them. The thought-emphasis, let us say, is the precursor of the act-emphasis. The singer without the score knows how vital this anticipatory process is; unless he feels what is coming ahead of the given moment, unless he can *mentally* sing the phrases slightly in advance of the vocal utterances, he is liable to come to a standstill. In fact, this running of the mental singing ahead of the vocal may account for the usual difficulty in ensemble music — the tendency to quicken instead of slacken the tempo, unless, of course, the selection is specially heavy. Triplett and Sanford found this tendency in scanning nursery rhymes, while Bolton discovered an opposite tendency in subjective rhythmization of uniform auditory impressions.

It is especially gratifying to note that the 'action' investigated by the author is what it purports to be, 'rhythmic,' in the precise and only meaning of the term. It is necessary to remark this, inasmuch as there is a recent tendency to unwarrantably extend the field of the rhythmical consciousness. Perhaps this can be traced back to Lanier. I cannot feel that there is the slightest justification for accepting and perpetuating his 'primary rhythm.' Mere regular recurrences are nothing but periodicities. The periodicity-consciousness is fundamentally different from the rhythm-consciousness, though it underlies the latter — I will not say, is its *absolute basis*, for rhythmical beats allow a certain percentage of variation, while a periodicity that is not absolutely periodic is a misnomer. Even a manner of grouping is possible, *e. g.*, by means of pauses, without occasioning rhythmical intuition. The latter arises only when the constituents of the groups are subordinated intensity-wise to a major beat (the centroid): on our innervation theory this process is closely related to, if not entirely dependent

upon, intensity changes in the functioning of the general musculature, though I believe the special connection is with the auditory apparatus. The onlooker of the dance does not feel the rhythm; it is a cold, pale 'ghost' to the dancers themselves until sounds (music) give it 'body' and 'warmth.' At any rate, the minimum motor in rhythm is the mental thud, which I usually localize where James localizes the feeling of the self—a view I have already expounded (*Rhythm of Speech*—centroid theory). Wundt, it seems to me, fathered this motor conception of rhythm; Bolton unwittingly commits himself to it; and it has been reformulated by Stetson and MacDougall and especially developed by Miner—but the latter, strangely as I regard it, apparently holds that to define rhythm as a coördination of coördinate elements is not inconsistent with the theory. Apart from the fact that such coördination is not the rule in listening to objectively homogeneous clicks, that the 'spondees' in natural speech are impossible to maintain, except for short stretches, that the distinction between the accented and non-accented notes is of the very 'structure' of the musical measure, and that arrhythmical movements tend to alternate regularly in intensity (Miyake), the theory, if true, would warrant the conclusion that rhythmical experience—I am almost tempted to say feeling, for I believe, with MacDougall, that there must be at least a rudiment of affective tone present—consists in a coördination of subordinated units. The first order of units, *i. e.*, the units within the unities or groups, always manifest the phenomenon of subordination; the second order, *i. e.*, the unities, frequently, especially in verse and song. On this distinction we obtain two rhythms, perhaps equally 'primary,' especially viewed genetically: (1) Coördinated simple subordinations (first order); (2) coördinated complex subordinations (first plus second order). The coördination appertains to time, the subordination to intensity.

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#### HABIT.

*Habit.* B. R. ANDREWS. *Am. Journ. of Psy.*, XIV., pp. 121-150.

Habit, defined from the standpoint of general psychology, is the 'mode of mental functioning in which repeated processes are in consciousness.' Hitherto the most adequate discussions, the writer points out, have been made by the functional psychologists, James, Sully and Stout. Külpe treats habit from the structural point of view but his analysis is incomplete. The writer, making his inquiry from the structural standpoint, finds certain differences of pattern between habitual and non-habitual states of consciousness. In the former, the

processes are meager, uniformly indistinct, and so closely connected that one term unconsciously brings on the next. In the latter, the processes are of two degrees of clearness, distinct and indistinct (those toward which attention is directed are distinct) and are joined together by conscious selection. A mood of familiarity or one of indifference is the distinguishing mark in habitual functioning, while non-habitual functioning is characterized by the consciousness of effort directed to the series of movements necessary to attain the end in view.

According as habits manifest themselves in the reappearance of specific experiences or in the shaping of new processes, we have specific or general habits. A further classification can be made into Titchener's levels to indicate the varying degree of persistence and influence on consciousness of habits according as they are due to the recency, intensity, or repetition of an occurrence.

The development of habit is explained by the author from the physiological standpoint. Habit is even spoken of as 'at bottom a physiological phenomenon.' Objection might be made to this and to other loose statements of a similar character. Nothing else, however, seems to be meant but the ordinary parallelism.

The article, as a whole, is a clear and concise discussion of the subject but it brings out nothing new.

WINIFRED HYDE.

BRYN MAWR COLLEGE.

#### MUSCLE AND WORK.

*Ueber Muskelzustände.* KONRAD RIEGER. *Zeitschr. f. Psych. und Phys. d. Sinnesorgane*, Band 31, S. 1-46, und Band 32, S. 377-415.

In this series of papers the author presents the results in part of his studies upon muscle conditions. He has pointed out a great variety of phenomena that are almost entirely new and must prove of immense importance in future work upon muscular activity and fatigue. The author has taken exceptional pains, sometimes perhaps too much, to make himself clear and to show the bearings of the phenomena he studies. His first proposition is that muscles are elastic bands whose contractile force is a function alone of their length and temperature. The work divides itself into three chapters. In the first he studies the elastic force as affected by length alone, in the second the time element in relation to the way length affects contractile force, and in the third the influence of temperature. With respect to length the first conclusion is that the muscular arrangements in the body are such that in



turning members about joints the muscles do not act most strongly when they are short and thus do not lose too much of their natural elasticity, which is great when they are long, and small when they are short. Muscles keep with equal stretching moments their equilibrium better and a longer time when they are long than when they are short. The further inquiry relates to the way in which the elastic force of a muscle acts as a brake, forming what is called the muscle brake. The movements of the lower leg about the knee joint are studied in relation to that of a wooden lever acting in the same way and to the linear stretching of a rubber band. The lever is fastened at one end and weighted. This weight plus the heft of the lever is counterbalanced by a second weight attached to a string that runs over a pulley near the ceiling of the room. Small additions and subtractions are made to or from the counterbalance and the positions assumed by the lever are indicated upon a smoked drum. A similar arrangement for the lower leg is given and the corresponding positions assumed by the member under the influence of equal additions or subtractions. The two figures upon the drum are strikingly different, and this our author attributes to the influence of the muscle elasticity acting as a brake. The figure made by a rubber band stretched by a weight to or from which additions and subtractions of equally graduated weights are made is with minor details very similar to that made by the leg. The organic elasticity shows itself more clearly in the muscle group than in the linear expansion of a rubber band. In dealing with the lever and lower limb seven weights of 200 grams each are taken away in order and then returned at equal intervals. The excursions of the lever grow less with each subtraction and then increase with about the same amount as the additions are made. With the muscles the excursions become greater with each subtraction, and when the additions are made the excursions are first very small, become greater and are greatest with the addition of the fourth weight. The first phenomenon is attributed to the force which the muscle group possesses alone as a result of its longer length in comparison to its short condition and the second to what is called elastic after-effect and fatigue. In this respect the rubber band and the muscle differ most from one another; the two do not act alike after stretching, and this is to be found chiefly in the effect of the antagonistic muscles. The point of interest is that the elastic contractility of both muscle and rubber band is different according as the previous condition of both muscle and rubber band has been long or short. The increase or decrease of elastic power will be great or small according as these previous conditions have persisted longer or shorter periods of time.

He next deals with movements that take place with or without elastic recoil. Movements are of two kinds, those that return to a former condition of the distribution of forces by means of elastic recoil and those that assume a new condition and a new position by a lasting distribution of their forces without elastic recoil. This concerns only rapid movements, for in slow movements an interference altering the distribution of forces is at any time possible in the course of the movement. A rapid movement is one that exceeds three strokes in a single direction or two backward and forward movements in the second. By the employment of elastic recoil eight or at most nine backward and forward movements are possible in the second. If then one wishes to attain considerable rapidity of movement, one must move with the continuous employment of elastic recoil. It may be said that before the beginning of a movement it must be decided whether elastic recoil is to be used or not. This need not, however, be a conscious intention. The chief interest centers about the method of bringing about a standstill after a rapid movement. This may be accomplished in three different ways, first by striking against a buffer, second when the turning moment of the member is equaled by gravity, and third by the muscle brake. The important question relates to the stopping of a movement where there is no external resistance that can be likened to a buffer. A rapid motion cannot be brought to a standstill without continuation or recoil. "If one sets before himself simply to pass from one position into a new one without external resistance, then he does it naturally not at the maximal rapidity but simply in a tempo so measured that the elastic recoil needs no especial suppression." If the movement is to be arrested by the muscle brake, "this exact balancing of the elastic forces which is necessary in a free excursion demands great nervous effort. And it is now certainly very clear that the motory machinery seeks always as far as possible to avoid this nervous effort."

The practical value of all this is seen in the writing of the different letters of the alphabet. If one makes a backward and forward motion without elastic recoil (a wavy line), two full movements a second only are possible, but if made with elastic recoil, then six to nine complete movements become possible. The loss of time comes about through the interruptions in which the elastic recoil is suppressed. The making of a point or dot, therefore, requires more time than a line. The letter *a* which covers a greater linear extent than the letter *r* can be made in less time and with greater accuracy because it can be made with the employment of elastic recoil. *M*, which is one of the longest letters in the alphabet, may be made more quickly than

most others for the same reason as named above. The temporal relations of movements that are made, now with, and now without, elastic recoil is next taken up. A motion that can be interrupted at any time is under constant nervous control and therefore is without elastic recoil. Such a motion cannot much exceed a rate of two a second, but it can be interrupted for any length of time that is desired. A faster backward and forward motion than this is made with elastic recoil and cannot be interrupted without sufficient time to allow a redistribution of the forces through the nerves. The least amount of time in which this can be done amounts to about one third of a second. The rapid motion with interruptions is on this account no faster than the slow one. If the interruption be made not through the muscle brake but by striking against a firm resistance, a shorter pause will suffice in which to take up the motion again. This long pause, we are told, 'is necessary for the disappearance of the increase in elastic force through which the elastic recoil has been suppressed.'

This notice covers only the first two parts. The work has been most thoroughly done and before publication most carefully and deliberately thought out as the subject demands. The author has found little or no assistance at all from a painstaking search of the literature upon the subject of muscle and nerve activity. It is, therefore, a piece of real pioneering throughout. Its importance for the subject of writing and all forms of manual exercise where the economy of effort is a consideration must finally prove very great.

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*Validity of the Ergograph as a Measurer of Work Capacity.*

THADDEUS L. BOLTON and ELEONORA T. MILLER. University of Nebraska Studies, January, 1904.

"The particular purpose of this experiment can be definitely stated in the form of a question the solution of which has been attempted. Can the ergograph be used as a measurer of work capacity; and, if so, under what conditions and within what limitations will its validity be greatest?"

The experiment was performed by two observers and continued over a period of more than three months. The ergograph used was one which employed the flexion of the middle finger, a modified form of that designed by Dr. Hoch in Professor Kraepelin's laboratory. The conclusions drawn by the writers are as follows: "Ergograph records change relatively in the course of a long series and thus the first records in a series are invalidated, for maximum performances

furnish a more reliable measure of work capacity. Since exercise induces a condition within the muscles themselves which reduces their capacity for work, ergograph records have slight validity until inurement has become thorough and coördination complete. The ergograph is quite unadapted to the obtaining of exact statistics upon a large number of individuals. A few records taken upon unpracticed subjects, both before and after operations whose influences are thought to affect muscular power, are without the slightest claims to trustworthiness. Influences which are supposed to effect the ergograph records must be studied by the way they affect practice gain. Practice gain with the ergograph is due to changes in the direction of inurement, coördination, rhythm, and endurance in muscular power. Practice gain as shown by the ergograph is practically unlimited. (This means, probably, that no observer would have sufficient perseverance to reach the limit.) Fatigue is a necessary condition for practice gain. It is essential to growth. (It is doubtful whether the experiment proves this.) Practice gain means increased power to recuperate. Vicarious practice effect (*i. e.*, the effect of practice by one hand on the corresponding finger of the other) is chiefly in the direction of coördination and rhythm."

The writers deserve credit for the care and patience with which they have performed an experiment of great difficulty. It is not clear from the tables what is meant by the expression 'average performance increase.' A bibliography is appended.

GRACE HELEN KENT.

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#### PSYCHOLOGICAL METHODS.

*Die Gesichtspunkte und die Thatsachen der psychophysischen Methodik.* G. E. MÜLLER, Göttingen. (Sep.-Abd. aus Ergebnisse der Physiologie, II. Jahrgang, II. Abteilung.) Wiesbaden, J. F. Bergmann, 1904. Pp. 252.

This is a compendium, or manual, of psychophysics from the hands of a pioneer and master. The treatment is historical, critical and constructive, with the aim of presenting a comprehensive view of the whole field, reducing the data to their lowest terms, and filling in gaps with reference to points of view and technique of method. Müller accepts the four usually recognized cases to which psychophysics methods apply; namely, the absolute threshold, the threshold of difference, equivalent stimuli, and equivalent difference. But he launches a new classification of the methods employed in the measur-

ing of these. The methods are reduced to three general classes: (1) the production method, (2) the limit-method (*Grenzmethode*), and (3) the method of constant stimuli—the constant-method. This is a good stroke in the direction of simplification, but the work of subdividing and correlating naturally leads to difficulties which no single writer can overcome. The adoption of this classification is not incompatible with the retaining of some of the old names for methods; and if the avowed aim to enable the future investigator to report his method by simply naming it, shall be realized, a more systematic arrangement of subdivisions must be made. Some varieties of methods may well bear the name of the originator, following the analogy of botany. The critical part of the work is the most valuable and will do much to clear up errors. A detailed review of the formulæ, conditions of experiment, program, etc., would not interest the general reader; and for the laboratory psychologist it is not necessary, because he will peruse this work as a welcome and indispensable guide. Each master of some detail may amplify and correct the position taken in this text, for the appearance of this work marks a vantage-ground in psychology; and, as we have been in the habit of going back to Fechner—for many purposes hereafter we may go back to Müller.

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*Sur la valeur des questionnaires en psychologie.* TH. RIBOT.  
Journal de Psychologie, January-February, 1904.

The questionnaire is here discussed as a psychological method intermediate between observation and experiment. It appears to have originated about 1880 with Galton in his *Inquiry into Human Faculty*. There are two forms: the indirect inquiry or the questionnaire properly so-called; the direct or oral. The first comes under the head of *variatio et translatio experimenti* as demanded by the rules of method. Unfortunately the results are disappointing. The scheme fails from the nature of the subject proposed or from the fault of the public to which it is addressed. It is too complex or too delicate to be divided up and expressed in terms so clear as to bring only precise answers. By the questionnaire we can get at a person's income but not his temperament, his religion but not his intellectual type. Thus Charcot's enquiry in heredity, though based on from 1,200 to 1,500 examples from a well-disposed public, brought returns which left an exceedingly vague impression; so the questionnaire of the Society for Psychical Research as to telepathy and veridical hallucinations, failing to treat of



the honesty of those questioned, simply showed that illusions were possible. Although larger subjects, in which passionate feelings may be evoked, are interdicted, the questionnaire has been of service in smaller matters. Yet here there is often a descent into puerilities in which Americans have been especial offenders.

The difficulties of the indirect inquiry include: (1) The veracity of the respondents. Here there is a too theoretical trust in human nature. The desire to be sincere is not enough to make one sincere. The problem of disposition brings up the great question of testimony and the determination of its value. (2) The competence of the respondents. As to the questionnaires spread by the journals and reviews, those who answer are often of an abnormal type, persons possessed with a desire to confess, but incapable by nature of giving information of a standard value.

To Ribot direct oral interrogation alone appears credible and profitable to psychology. Yet it has its drawbacks. It is necessarily limited and cannot reach large masses. Besides the difficulty of constant journeys it demands a close knowledge of the subject, of his general education, social status, habits, stamp of mind, and intellectual culture both general and special. Ignorance of all these factors puts us anew in the region of the unknown. Next to be considered is the interpretation of the answers by the psychologist. Here enters the inevitable personal equation, whereas the attitude of the interrogator should be receptive and passive; his chief virtue would be a resemblance to a registering instrument.

The writer now makes a plea for the drawing up of an inventory of all the questions which have been treated by means of the questionnaire. The delicate part of this task would be, he says, to strike a balance of the results obtained or at least probable. This work of criticism is of capital importance and yet generally neglected. Questionnaires published in one psychological journal are apt to be copied in others in a merely mechanical way. Their worth seems to depend upon their number. But this is too much like the method of the referendum, an application of universal suffrage to the problems of psychology.

The oral and not the statistical inquiry has regard to quality. The methodical study of ten persons by ten psychologists has incomparably more value than the gathering of hundreds of signed or unsigned papers. So Stern proposes a working organization of professional and amateur psychologists. Centers may be found in laboratories, universities, schools, periodical congresses, and special meetings which are in touch with the public. To this scheme Ribot in conclusion

makes one suggestion: that it is not desirable to confine ourselves to a single category of individuals (for example students who serve almost always as subjects) else our conclusions will be narrow. In fine, Ribot's attitude appears to be that the ordinary questionnaire is questionable. He remarks in a footnote that he cannot here examine the alternative scheme — the more recent method of 'tests.'

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*Auditory Tests.* BENJAMIN RICHARD ANDREWS, A.B., A.M., Am. Journal of Psychol., XV., No. 1, pp. 14-56.

The present paper falls into three parts: (1) The concept of mental tests and their significance for psychology; (2) a review of American literature, and (3) tests of acuity of hearing.

The thesis of part I. is "Mental tests, as tests, are not to be regarded as part of psychology, neither should direct contributions to psychology be expected from them."

The arguments supporting this thesis are: (1) Mental tests are not related to structural psychology, since the purpose of the tests is to reveal the significance of a mental characteristic for the possessor. Pure psychology does not recognize the individuals as such. (2) Mental tests are not related to functional psychology, since the latter does not recognize the practical purpose of these tests. "Mental tests, therefore, may be considered as an application of psychology."

The discussion of the literature does no more than indicate the trend of American work in mental anthropometry. Allusion is made to the frequent confusion between psychology and mental anthropometry, and to the unjustifiableness of anticipating contributions to psychology from the results of mental tests.

The third part of the paper is a contribution to mental anthropometry in the field of audition.

After emphasizing the important function of hearing conversational speech the author outlines a method of testing acuity of hearing speech sounds. Two conditions should control the selection of test words: (a) the words should be of 'equal apperceptive value'; (b) they should include the various elementary sounds, both vowels and consonants. Number words from one to ninety-nine were chosen. To determine the extreme range of hearing, the words were spoken at varying distances from the observer until the threshold of audition was found (by the method of minimal change). The author attributes more value to the *method of degree of accuracy*. One or more constant ranges are chosen at which a list of one hundred words is

spoken, the percentage of accurate audition furnishing the results, *i. e.*, the number of right cases at a given distance and under a given set of conditions is the basis of computation.

In an appendix is found an 'experimental examination of speech methods.'

Tests for the determination of extreme range showed (1) "there are differences in the ease of audition for the different number of syllables; although (2) these differences are not constant throughout the various ranges."

The method of degree of accuracy was tested in further experiments. The value of the method lies in the ability to compare the acuity of various observers under any one set of conditions. A method of standardizing the results was not found.

The main points of Mr. Andrews' work are: (1) The distinction between psychology and mental anthropometry is rendered explicit; (2) the method of degree of accuracy is applied to distinguish degrees of acuity of hearing in any group of individuals.

The article will be concluded with tests of musical capacity, and tests used in diagnosis of aural disease.

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#### VISION.

*Ueber das Helligkeitsverhältnis monokular und binokular ausgelöster Lichtempfindungen.* (Fortsetzung der Untersuchungen über Dunkeladaptation des Sehorganes.) H. PIPER. *Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg.*, 1903, XXXII., 161-176.

The author's problem is to determine whether in binocular vision the intensities of the two visual processes are fused, or combined, into a single resultant greater than its two components. Do we see an object with two eyes as brighter than we do with one? Does the binocular brightness-effect represent any kind of a summation process?

The views and statements of Fechner, Aubert, Helmholtz, Hering and Schenck are cited. These show no agreement on the point in question, though the consensus of results seems to favor the summation hypothesis. The author's previous investigation on the comparative threshold values of the retina when adapted to light and darkness, had indicated that such a summation of intensities was not present when the eyes were adapted to the light, while on the contrary, when the eyes were completely adapted to the dark for a period of ten to fifteen minutes, the binocular intensity was nearly double that of monocular vision, although the objective stimulus was the same.

In essentials, the device used was a light placed before two opalescent glass windows of the same size and transparent power. Between the light and each window was a smaller glass provided with an iris-shutter device, whose diameter could be read off in millimeters. Where the two diameters were equal, the brightnesses of the two windows were identical, and when unequal, the ratio of the two brightnesses was given by the square of the ratio between the two diameters. The observer was stationed before the two windows in an otherwise dark room. The position of the head was so arranged that either window could be viewed binocularly and the other monocularly. In comparisons the eyes were allowed to change their fixation from one window to the other as desired. In this way binocular vision was compared with monocular vision of the right and left eye respectively, while the objective stimuli were identical in intensity. In case the monocular brightness was judged less, its objective stimulus was intensified by the shutter device until the two brightnesses were judged to be equal. In this way brightness-differences between binocular and monocular vision could be stated in quantitative terms. Where there was no brightness-difference with the same objective intensity, the judgment was checked by starting with different brightnesses, and varying the objective intensities until a judgment of 'no difference' was reached.

The main conclusions are: (1) With the eyes adapted to the light, there is no brightness summation in binocular vision. Objects seen with two eyes are not brighter than when seen by either eye alone. (2) With eyes adapted to the dark, summation is always present. With the objective stimuli equal in intensity, the binocular brightness is much greater than that of either eye alone. The ratio as quantitatively determined was very constant for variable objective intensities, and for different observers. The average result was from 1.6 to 1.7. When the objective light intensity was extremely small, the ratio approximated 2.

The results thus confirmed the inferences of the author's previous investigation. The work seems to have been carefully done, and all objections to the method are candidly considered.

The results of (1) directly contradict those of Aubert and Fechner. Experiments now being conducted in the Chicago Laboratory indicate pretty conclusively that this discrepancy is due to the different methods used, and that Piper's method and results are to be regarded as the more reliable.

HARVEY CARR.

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*Wie verhalten sich die Helmholtzschen Grundfarben zur Weite der Pupille?* GISELA SCHÄFER, Zeitsch. f. Psychol. u. Physiol. der Sinnesorgane, XXXII, 416.

Dr. Schäfer refers first to the previous work of Sachs and Abelsdorff who, experimenting with pigmented papers and monochromatic lights, reached the conclusion that the width of the pupil is proportional to the brightness of the stimulation.

The author's attention was caught by the fact that very saturated colors, even when apparently of unlike brightness, produce a dazzling sensation. Are the colors behaving in this way the ground colors of Helmholtz?

Dr. Schäfer proposed to test this fact by measuring the pupillary reaction. On a field of given size white was produced by the mixture of two complementary colors and observed with a definite portion of the retina. If one of the colors is then removed, the pupil is enlarged. Is this reaction essentially weaker when the remaining color is a ground color, than when it is not? (The width of the pupil was measured by means of a diffusion circle which a point of light projected into the same eye that was used in the observation of the mixed field.)

The author's results are as follows: Green has always the greatest diffusion circle, red next and the mixture of the two the least. This holds true even when the red light is so reduced in intensity that it no longer neutralizes the green. Violet always gives a greater diffusion circle than yellow, and again the mixture of the two the least. This is likewise true when the yellow is so reduced in intensity that it no longer neutralizes the blue. White produced from the mixture of red and green gives a less diffusion circle than the white made from a mixture of violet and yellow.

Since the ground color red produces a stronger pupillo-motor effect than its complement, but with violet this condition is reversed, the author concludes that ground colors as such exert no especially prominent pupillo-motor effects.

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### SPACE PERCEPTION.

*Beiträge zur Physiologie des Raumsinns.* Dritter Teil. Täuschungen in der Wahrnehmung der Richtungen durch das Ohr labyrinth. E. v. CRYN. Pflüger's Archiv, 1903, XCIV., 139-250.

The problem for investigation is threefold: (1) To discover the illusions of direction as perceived through the labyrinth of the ear, (2) to measure these illusions when found, and (3) to point out the bear-



ing the illusions of one direction have upon the determination of the other two fundamental directions. The observer was supplied with a pencil and ruler with which he was to draw the lines of the three fundamental directions upon sheets of paper fastened in a vertical or horizontal position according to instructions. This was done both in ordinary daylight and in a dark room where the observer was also blindfolded.

The first series, in which the observers have head and body erect, gives two types of results: (1) The directions drawn deviate from the normal and make an angle of intersection of about  $90^\circ$ . (2) One of the directions drawn is practically correct, but the other direction varies whereby the angle of intersection deviates from  $90^\circ$ . Untrained observers produce the first type, while trained observers produce the second type. These illusions are constant with each individual.

Then the effect of turning the head about the sagittal axis, *i. e.*, inclining the head to the right or left shoulder, was examined. The apparent vertical axis was found to incline in the direction opposite to that of the inclination of the head, and likewise the apparent transverse axis. Thus, if the head was leaned toward the right shoulder the vertical axis was drawn leaning toward the left.

The experiments upon the illusions in turning the head about the vertical and horizontal axes show that the turning upon the vertical axis scarcely at all disturbs the ordinary perception of the vertical direction and the turning upon the transverse axis disturbs only very slightly. In a similar manner it was found that the turning of the head about the sagittal axis involves the same illusions in the sagittal direction as in the vertical direction.

The next series of experiments was undertaken to determine the effect that the position of the eyeballs has upon the determination of any given direction. Here it was found that the position of the eyeballs does not change the quality of the illusions as they ordinarily occurred under the preceding conditions, but the strength of the illusions in the horizontal direction was somewhat increased. It was noticed that the illusions were considerably larger in one observer who was accustomed to take his regular practice on his violin just before giving his observations in these experiments. This led to a further investigation of the effect of sound stimulation upon these illusions. It was found that they are increased after sound stimulation. From this von Cyon makes two inferences: First, this furnishes a simple proof that the illusions here considered are based upon the perception of the fundamental directions by the labyrinth; and second, it demonstrates that the vestibular nerves to which the perception of direction

is due, can be stimulated by sound waves, *i. e.*, by the same stimuli as the auditory nerves. These inferences were further corroborated by a brief study of the illusions in the localization of sound. These were found to be analogous to the other illusions even in the peculiarities of individuals. The last series is a study of the perception of parallel directions. The observer was blindfolded in a dark room in which there was a table whose position the observer knew. He was to walk toward the table so that his right-left axis would be parallel to the edge of the table. Right-handed persons tend to turn to the right and left-handed persons tend to turn to the left. These two tendencies are due to the difference in the position of the head and body in the two types of people. In evaluating the results the author seems to have overlooked the element of muscle sense in drawing the directions. This article completes an investigation of the problem of space perception which von Cyon has carried on for over thirty years.

DANIEL STARCH.

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*Sur l'Apperception des Différences Tactiles.* L. MARILLIER et J. PHILIPPE. *Revue Philosophique*, 1903, LVI., 619-627.

Professor James, in the *Principles of Psychology*, II., ch. XX., p. 170, describes certain tests that lead to the following conclusions regarding tactile discrimination.

With nine or ten subjects he ascertained what difference it made in the discrimination of two points to have them alike or unlike. The contact-points used were: (a) two large needle heads; (b) two screw heads; and (c) a needle head and a screw head. When the points gave different qualities of feeling (as in c), the discrimination was facilitated some but often was not perceptible 'twenty times running.' However, if a rotary movement was given to one of the points, discrimination was easier. On the whole, Professor James concludes that the likeness or the difference of the forms of the contact-points has little effect.

In the article under consideration, L. Marillier and Dr. J. Philippe give a popular account of more extensive experiments of a similar character, reported in detail in the *Journal de Physiologie et de Pathologie générale*, janv., 1903, which lead them to adopt a somewhat different view.

The contact-points used by them were two spheres 1 mm. in diameter; two cylinders 1 mm. in diameter, and two triangular prisms 1 mm. on a side; all of ivory, and sufficiently alike in area and temperature to make the difference in form the only difference.

To secure records from typically different parts of the body, they made their tests along the entire length of six longitudinal and approximately parallel lines drawn down the body from the shoulders, three in front and three behind; and along two lines drawn over the front surface and back surface of each arm respectively.

They find that the threshold for discrimination is almost always lower when the skin is touched with two points differing in form as compared with that for similar points.

Moreover, they conclude that the discrimination becomes easier the more different the contacts and that the effect of difference of form grows greater with age. With young subjects the threshold for different contacts is about two-thirds and with adult subjects about one-half of that for similar points.

The interpretation of these facts is to be found in the development of an apperception of form by means of tactile sensations. Whether the development is independent of vision, or takes place in coöperation with or under its direction, cannot be determined. At any rate, the authors conclude we are not so insensible to form as Professor James was disposed to believe.

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## DISCUSSION AND CORRESPONDENCE.

### ACCOMMODATION AND CONVERGENCE. — A REPLY.

A recent criticism (*The Journal of Philosophy, Psychology and Scientific Methods*, I., No. 7, 180-181) of my review of Baird requires a word.

The writer points out that "the review imagines that I accept 'nativism as respects relative binocular localizations, while rejecting it for monocular vision.'" In the criticism he rejects it '*in toto*.' My conclusion was based on the following explicit statements in the original article (pp. 197-198): "It is in dealing with the phenomena of *relative* localization in binocular vision that Nativism has been most successful." "However creditably Hering's theory may have acquitted itself as regards the binocular estimation of relative distance, its defects become apparent as soon as it passes beyond this narrow field. Its account of the binocular estimation of *absolute* [why not also *relative*?] distance, and its explanation of the phenomena both of *absolute* and *relative* localization in *monocular* vision, must be rejected" (italics all mine). Plainly, my statement is justified.

Again, for years I have been not unfamiliar with the theories of 'Hillebrand, Arrer and Dixon' and with the problems of 'psychological space' in general; what is more, I do not touch upon them in the review. It is through misconception of one of my statements that I am said to misconceive them, viz., 'What the paper gives is a confirmation of substantially all the *experimental results as such*' (italics mine). I explicitly referred, not to opinions, deductions, theoretical conclusions, but to 'experimental results as such.' I distinguish between the experimental data and results and the guesses and theories deduced from them. That my statement is correct as it stands (there are certain differences) the writer's own statements seem to indicate (cf. the article, *e. g.*, 148).

Third, that the apparatus was 'substantially the same,' I find reason to reiterate: I find no difference in principle, and interpret the writer in accordance with this position. But witness how at variance his statements are. (1) *In the criticism*: "It is no less clear from the literature that the various forms of apparatus which have been employed in the investigation of the problem are *essentially* different in *principle*." "It is manifestly impossible that my apparatus could have been 'substantially the same' as another which differed *radically* from it in principle"; (2) *in the article*: "The form of apparatus devised by Hillebrand seems to meet the requirements of the present investigation." "To Hillebrand, then, we are indebted for the apparatus. We have *modified* it only in *non-essential details*" (italics all mine). The review was not unmindful of certain differences; it regarded them as less important.

Fourth, owing to a misconception the reviewer is said to 'utterly' fail to 'understand the construction of the apparatus.' It appears from the criticism that he believed the eye fixated a 'black patch' — 'at a greater distance than a white'; as a matter of fact no such statement was made. What I said was: 'The eye localizes a black patch (or what not) at a greater distance than a white.' The statement is justified by results from the Chicago Laboratory (PSYCH. REV., V., 595 f.), and by my own (a chapter in a forthcoming work, *Optical Illusions of Reversible Perspective*). I indicated definitely what the character of the fixation object was: 'to fixate a line' — the word *abutting* might be preferred to 'overlapping.' The author refers to it as 'screen-edge,' and calls special attention to the fact that half the field of vision was *black*, half *white* (p. 173). The situation I hinted at is one not 'under control' in the experiment.

Fifth, my use of the phrase 'range of accommodation' was in the sense in which Sanford employs line of accommodation (*Experimental*

*Psychology*, p. 93). It is obvious that the discrimination limen must be larger for the merely accommodating eye, for a given degree of accommodation is sufficient for a certain extent of the line of sight. I submitted that the line of accommodation may be one factor accounting for the larger monocular limen actually found in the experiment.

Finally, respecting the value of my suggestions for future research, I need only add that it is judicious to suspend judgment until they have been subjected to rigorous test. I submit that the problem has not been closed for all time and that it has not yet reached its final stage of development.

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TO THE EDITOR OF THE PSYCHOLOGICAL REVIEW.

*Sir:* Our attention has recently been called to an article entitled 'Personal Sources of Christian Science,' by Prof. I. Woodbridge Riley, of the University of New Brunswick, Fredericton, Canada, published in the November issue of the PSYCHOLOGICAL REVIEW. In reading this able article we cannot help feeling that the Professor's talents are worthy of a higher use than that of attempting to disprove the authority of Christian science and the genuineness of the work of its discoverer and founder.

Without entering into the arena of public controversy, or attempting to take up the sword of hostile argument, we will relate some of the things that Christian science has done and is doing in our very midst, and then leave the decision of its merits to the judgment of the impartial reader.

The world has had many religions, philosophies and systems of therapeutics, both physical and metaphysical, all of which when weighed in the balance have been found wanting in some respects, and we ask — is there any fairer standard by which to judge their relative values than that injunction of the Holy Scriptures 'by their fruits ye shall know them'?

The writer's first impressions of Christian science were gathered from reading Mrs. Eddy's book *Science and Health with Key to the Scriptures*, in the early part of 1892, at the time he was suffering from a disordered liver and chronic dyspepsia, which had defied skillful medical aid. About three weeks after the commencement of this study a distinct physical improvement was remarked, and although there was no help received outside of the reading of Mrs. Eddy's book, this healing influence continued until every symptom of the old diseases had completely vanished. This wholly unexpected recovery created a



desire for a better understanding of the law governing this physical phenomenon, and a close study of this great metaphysical principle during the past twelve years has conclusively proved its efficacy under all conditions where circumstances make its demonstration possible.

Any intellectual effort to contradict Christian science by theoretical argument, no matter how able and scholarly the attempt may be, will tend to emphasize the vast difference between the wisdom of man and the divine Intelligence, and the gulf existing between them can only be bridged over by yielding the former in favor of the latter.

The repeated attempts to endow the illiterate and materialistic P. P. Quimby with the credit of having originated Christian science are too far fetched for serious consideration. No claim has ever been established in his behalf which can be considered in the slightest degree tangible. In criticising Mrs. Eddy's writings it would have been, in our judgment, fairer to have quoted from her later editions, instead of from the first, which embodied her early effort to state a subject at once novel and abstruse. If the professor would look for the good in Mrs. Eddy's life and philosophy, we feel assured that he would find so much that his pen would not again be used in the attempt to overthrow a system which is benefiting thousands of the human race. Professor Riley however makes an honest admission when he says, 'That Mrs. Eddy has been successful in treating many nervous, non-organic troubles cannot be denied.'

If a thing is good, would it not be wise to encourage its progress? If on the other hand it is not good, the best possible way to get rid of it is to offer something better, and public opinion will invariably decide in favor of that which is best. Instead of expending our useful and God-given ability in profitless discussion, which will do nobody any good, let us remember the Scriptural quotation — "The kingdom of God cometh not with observation; neither shall they say, Lo here! or lo there! for, behold, the kingdom of God is within you." (Luke 17: 20, 21.)

MARCH 15, 1904.

RICHARD P. VERRALL,  
*Christian Science Publication Committee,*  
*State of New York.*

#### BOOKS RECEIVED FROM APRIL 7 TO MAY 7.

*Ergographie de la Main droite et de la Main gauche.* H. SCHOUTEDEN. Brussels, Hayes, 1904. Pp. 28.

*Bibliography of the Published Writings of G. Stanley Hall.*  
Publ. Clark Univ. Library. Vol. I., No. 1, 1903. [196 titles,

beginning with 'Philanthropy,' poem delivered on Class Day at Williams College, June 17, 1867.]

*Bibliography of Child Study for the Year 1902.* LOUIS N. WILSON. Publ. of the Clark Univ. Library. Vol. I., No. 2, 1904. Worcester, Mass. [A continuation of Mr. Wilson's useful series. This issue contains 344 titles.]

*Sulla Filosofia monistica in Italia.* E. MORSELLI. Turin, Un. Tip. Ed., 1904. Pp. xliii. [Separate publication of the Introduction written by Prof. Morselli for the Italian translation of Haeckel's *Problemi dell' Universo*, now in press.]

*Le moderne Teorie del Genio.* R. NAZZARI. Rome, Tipog. Coop. Rom., 1904. Pp. ix + 172. L. 4.

*The New International Encyclopedia.* Vols. XV., XVI., XVII. (Rice to Zyrians). New York, Dodd, Mead & Co., 1904. [These volumes complete this great work.]

*Fatigue.* A. MOSSO. Tr. by M. and W. B. DRUMMOND. New York, Putnams; London, Sonnenschein, 1904. Pp. xiv + 334. [This seems to be a rendering of Mosso's text as originally published, without substantial alteration, although the author is said to have seen the proofs. It will no doubt find its use, though it is a pity the matter was not gone over by the author, in view of the large amount of work recently done on this subject. Such researches as those given in Binet's *Fatigue intellectuelle*, the new apparatus of various sorts which have been used in preference to Mosso's ergograph, and the later physiological researches into the conditions of toxic modification of the blood, — all of these things should be taken note of.]

It may be noticed that a book on fatigue is announced in Toulouse's *Bibl. de Psych. expér.*, and that there is a chapter giving literary citations on this subject in the recent work of Woodworth, *Mouvement*, in the same French series. It is very doubtful, therefore, whether — apart from its historical value and the high authority of its author — there is any real demand for an English version when the French edition is so accessible.

J. M. B.

*Descartes, Spinoza, and the New Philosophy.* J. IVERACH. World's Epoch-Makers. New York, Scribners (Importers), 1904. Pp. xii + 245. \$1.25.

*An Autobiography of Herbert Spencer.* 2 vols., illustrated. New York, Appletons, 1904. Pp. xv + 655 and ix + 613.

*The Psychological Index.* No. 10. (Bibliog. for 1903.) Issued by THE PSYCHOLOGICAL REVIEW, compiled by H. C. WARREN. Issued also as Bibliog. Supplement, No. 3, to the *Dict. of Philos. and Psychol.*, ed. by J. Mark Baldwin. New York, The Macmillan Co., 1904. 75 cts. [Contains 2,122 titles.]

*A Study in Reaction Time and Movement.* THOMAS V. MOORE. Monograph Suppl. No. 24 of THE PSYCHOLOGICAL REVIEW. New York, The Macmillan Co., 1904. Pp. iv + 86. 75 cents.

## NOTES AND NEWS.

PROFESSOR LLOYD MORGAN and Professor James Ward have been invited to attend the Jubilee celebration of the University of Wisconsin, June 5 to 9, and receive the honorary degree of LL.D. We understand that neither of them has been able to accept.

PROFESSOR GEORGE SANTAYANA, of Harvard, is to take his sabbatical year's leave of absence next year.

PROFESSOR C. S. SHERRINGTON, of Liverpool, delivered a course of ten lectures on the Silliman foundation at Yale, April 22 to May 2. The topic was 'Integrative Action by the Nervous System.'

THE EDITORS take pleasure in announcing that Professor Charles H. Judd, of Yale University, is to be hereafter associated with the REVIEW, being Editor of the new series of Monograph Supplements, which it is designed to make inter-university in character. As issued under Professor Judd's editorial management the monographs are to be considered 'supplementary' also to other journals which may care to adopt the plan. University departments are to have their productions issued, after the manner already begun in the *Harvard Psychological Studies*, in sub-series, the separate numbers of which may be bound together. This arrangement, it is hoped, will obviate in future the scattering of theses and other monographs in various university publications less accessible to the average reader. Fuller announcement may be expected in the next issue of the BULLETIN.

The following items are taken from the press:

A MEETING of experimental psychologists met for the reading of papers and discussion on the invitation of Professor Titchener, at Cornell University, on April 4 to 5.

PROFESSOR JOHN DEWEY is to spend the summer abroad. It is reported that he has been invited to a new chair in Philosophy founded at Columbia University by an anonymous donor.

MESSRS. GINN & Co. announce the posthumous publication of two volumes by the late Thomas Davidson: 'The Philosophy of Faust' and 'The Education of the Wage-Earners.'

THE *Revue de Métaphysique* announces a public subscription for a monument to the memory of Charles Renouvier. Subscriptions may be sent to Librairie Armand Colin, rue de Mézières, 5, Paris 6<sup>e</sup>, France. The sales of the volume *Charles Renouvier — Les derniers entretiens*, recueillis par Louis Prat (same publishers, price 2 fr. 50, and 8 fr.), are to be donated to this fund.

THE Oxford University Convocation has accepted the fund given by Pundit Krishnavarmá, a Balliol College man, for a Spencer Lectureship. It is interesting that both the Romanes and Spencer lectureships should be at Oxford. It is also reported that a memorial lectureship to the late Professor Robert Adamson is to be established at Owens College, Manchester. Subscriptions to this latter fund are received by Prof. S. Alexander (Owens College, Manchester, England).

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